The State of Computer Science Research in the U.S.
and
The Evolution of Federal Support for It

Statement of

Wm. A. Wulf, Ph.D.
President, National Academy of Engineering
and
AT&T Professor of Engineering and Applied Science, University of Virginia

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Good morning, Mr. Chairman and members of the committee. I am Wm. A. Wulf, president of the National Academy of Engineering, and on leave from being the AT&T Professor of Engineering and Applied Science in the Department of Computer Science at University of Virginia. I appreciate the opportunity to testify today on the state of Computer Science research in the U.S.

A few words about my background will provide a context for my remarks. I was a professor at Carnegie Mellon University (CMU) for 13 years (from 1968 to 1980); and during that time I did research in a number of subfields of Computer Science – specifically computer security, computer architecture, operating systems, programming languages, and optimizing compilers. I left CMU in 1980 to found and run a software company and subsequently served as an Assistant Director of the National Science Foundation (NSF). In 1991, I returned to academia at the University of Virginia, where I resumed my research in Computer Science. In 1997 I became President of the National Academy of Engineering which, together with the National Academy of Sciences, is chartered by the Congress to provide advice to the government on issues of science, engineering and health. Thus I have the fortunate perspective of being a recipient of federal research support, a witness to how that support translates into commercial product, someone with the responsibility of dispensing that research support, and a participant in a broad range of technology related public policy deliberations.

Before responding to the specifics of the questions in your letter inviting me here today, I would like to make four points concerning how I think about these issues.

First, in *Science the Endless Frontier*, the report that established our system of federal funding of basic research, Vannever Bush advocated a system in which the government funds research, but the research to be done is selected on its merit by the researchers themselves. He said that such a system would pay dividends to the nation in national security, prosperity, and health. It is hard to think of a better "poster child" for the truth of this assertion than Computer Science. Consider the abbreviated list:

> <u>National Security</u>: smart bombs, GPS, unprecedented "information awareness" for the war-fighter, unmanned robotic vehicles for surveillance, enormously enhanced training through use of virtual reality, etc
>
> <u>Prosperity</u>: a 3% national productivity growth fueled by information technology, dozens of multi-billion dollar per year industries (see Figure 1), internet-enabled business models, a 40-fold reduction in the cost of telephony, a global wireless phone system, etc.
>
> <u>Health</u>: Medical imagery (CAT scans, etc), cochlear implants, bio-sensors, smart prosthetics, smart defibrillating pacemakers, etc.

All of these were made possible by the federal investment in long-term, basic computing research. It is a mistake to think of such funding as an "expense"; it is an investment that

demonstrably has had a *huge* return! Technology such as that listed above is the return on the investments made a decade or more ago. Investments made today in research will have equally large returns for our children and grandchildren; conversely, it is our children and grandchildren that will pay if we do not make them now.

Second, computing and computer science is in the unusual position of being both a challenging intellectual discipline in itself, and providing an infrastructure for other fields of science, engineering, and commerce. While the benefits to society listed above can be directly attributed to computer science, there are also many more benefits that have resulted from the use of computing in everything from cosmology, to weather prediction, to health care, to Walmart's "just in time" inventory. Across this broad spectrum, computer science has enabled a better quality of life for us all. For me this simply reinforces the notion that funds expended on computing research are demonstrably investment, not expense. They are, in fact, an investment with an *enormous* multiplier because advances in computing and information technology have immediate, direct and tangible benefits on virtually all human activities.

Third, I do not believe the "linear model" of technology development! In my experience, the idea that basic research begets applied research begets development begets benefits to society is both wrong and counter-productive when applied to public policy decisions! Instead, there is a marvelously rich and productive interplay between basic scientific discovery and application, between universities and industry, between societal need and technology. We refer to Figure 1 as the "tiretracks chart"; it shows the relation between

industry and universities in the development of about twenty information technologies, each of which produces more than a billion dollars of revenue per year. As you can see, progress does not always start with basic research, and it often involves iteratively exchanging roles between university and industry. The bottom line, however, is that if federally-funded, university-based basic research weren't "in the loop", these enormously beneficial technologies would not exist. Basic research may not be the original source for all the benefits we enjoy from technology, but it is a vital and irreplaceable component of the rich system that produces them.
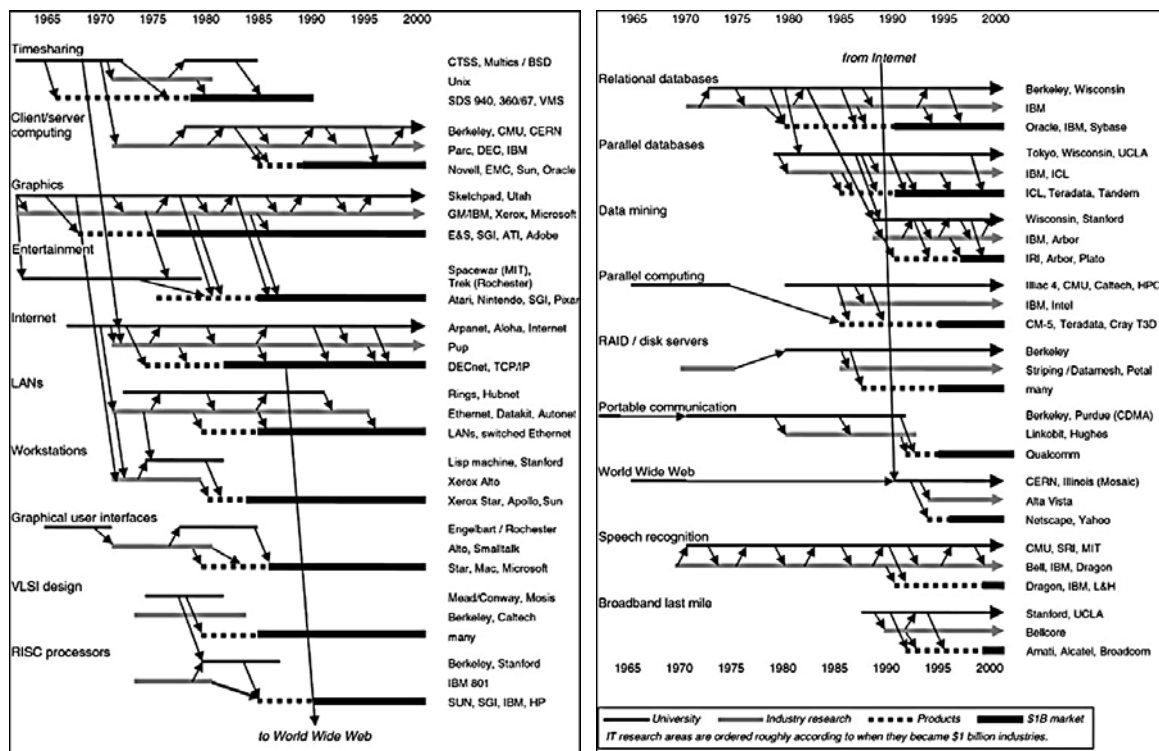

Figure 1:

Fourth and finally, it's about people, stupid! It is worth reminding ourselves that Bush's *Science The Endless Frontier* was written in response to President Roosevelt's question

about how we can ensure that, if there were another world war, we would have the *people* able to do what scientists and engineers did to help win WW II. For all the bounties that we can point to as coming from computing research, the most important output has been the cadre of educated women and men that can take us to the next level. From personal experience, I firmly believe that the U.S. early dominance in electronics and software was because of the students educated by the enlightened policies of DARPA and NSF beginning in the 1960's! If computing research has a large multiplier because of its broad application, then the people capable of doing that research are yet another multiplier on top of that! Disinvestment in university-based research is a disinvestment in the production of the next generation of people, with far greater negative impact than simply the loss of the research.

With that context, let me now turn to the three questions in your invitation to me:

1. What effects are shifts in federal support for computer science – e.g. shifts in the balance between short- and long-term research, shifts in roles of different agencies – having on academic and industrial computer science research? What effects are changes in the research likely to have on the future of the U.S. information technology industry and on innovation in the field?

2. Are the federal government's current priorities related to computer science research appropriate? If not, how should they be changed?

3. What are [my] views on the recent President's Information Technology Advisory Committee (PITAC) report on cybersecurity? What should the federal

government be doing to implement the recommendations of this report? Should

PITAC be renewed when its current term expires on June 1?

Although this hearing is about the state of computer science, I am concerned about what I

perceive as a shift to more risk averse funding of research in *all* of the physical sciences

and engineering, and in all of the agencies that have traditionally funded such research.

At a macro-level, I am concerned that while this committee has authorized a doubling of

the NSF budget, the funds have not been appropriated. I am equally concerned about the

proposed decrease of DoD 6.1 funding. It is easy to make, and even to understand, the

argument that in the current budget situation increases are not likely in either of these

accounts; nonetheless, I find it *deeply* troubling that there seems to be little recognition of

the long term consequences of a decision not to make these investments.

As I have testified to this committee before, it is *not* just that there is an increasingly

short-term focus in some agencies, it is that even in those agencies with a longer term

focus, when resources are tight, researchers themselves propose more incremental, less

risky projects. Where bold new ideas are needed, as in cybersecurity, we see

conservatism and temerity instead. There are exceptions of course, but perversely, when

resources are tight we generally get less out of what we do spend. Someone once said that

great research does not come from moments of great insight, but from moments of great

courage! When the existence of one's research program is on the line, courage becomes

even rarer than usual. There is a cascading effect of this – more timid PI's educate

students to be more timid, provoking a long term decline in the quality of research.

With respect to computer science within this general drift towards conservatism, I would make several points:

First, at NSF the budget for Computer and Information Science and Engineering (CISE) has grown nicely from when I ran it in the late 80's, and CISE is to be congratulated for using that growth to increase the average grant size rather than taking the politically easier route of funding more proposals. In addition, it has added center-scale projects through its Information Technology Research (ITR) program. Together, however, this has led to a potentially serious decline in the "success rate" in some areas – allthough the success rate is determined by a number of factors and I do not have access to the data to let me analyze just how serious this is in specific areas. What I *can* say from discussions with my colleagues is that the computer science community *believes* that it is serious and has adapted its behavior accordingly: more time is spent writing proposals, more failed proposals are "recycled", more incremental and less bold ideas are advanced, etc. I suspect that the decline in success rates is serious, but I *know* that even if it is not, it is having a significant negative impact.

NSF has, by the way, and with thanks to this committee, focused more resources on cybersecurity research. NSF is, in fact, now the major supporter of university-based research in this area. It is, however, also an example of the success rate problem mentioned above – only slightly more than 8% of the proposals in response to its Cyber Trust initiative were funded!

Having been the Assistant Director in charge of CISE, I can't help also remarking that there is often a misunderstanding of the CISE budget. About half of it is leverage for other fields, not computer science. CISE manages the Foundation's investment in cyber-infrastructure that is devoted to supporting those other fields. When at the Foundation I felt simultaneously proud to have the opportunity to leverage the success of those other fields, and frustrated at the misunderstanding by many of how little of our budget was actually devoted to the basic underpinning that created that leverage.

Second, I am deeply concerned about what has happened at DARPA. On top of a many year drift toward the less ambitious and more incremental, the Iraq war has been described as a reason to dramatically accelerate this – to focus on reaping the successes of the past, to focus on rapid development, to industrial development over university research, and to shift the balance strongly toward near term topics. While I can agree that reaping, developing and focusing on the near term *are* needed, so is long term investing. Without current investment there won't be anything to reap next time. Moreover, while there are many DoD organizations that can reap and develop, and that collectively have the bulk of DoD's Science and Technology budget, there was only one old-style DARPA, and it is gone.

The problem with trying to assess the consequences of the kind of shift we have seen at DARPA is that they are opportunity costs, measured in "might have beens", and at best evident only years after the fact. By comparison with the tangible, immediate results of reaping and developing, such costs may appear ephemeral and perhaps even wasteful.

Yet one can only wonder at what the world would be like today if the immediacy of the Viet Nam war had diverted ARPA from funding crazy ideas like networking, timesharing, VLSI, graphics, RISC architectures, RAID disk systems, parallel computing – or any number of other technologies that are *essential* to today's computer industry and whose results pay off daily to industry, government and the consumer *as well as* the military.

Any number of studies have shown that it takes about fifteen years, plus or minus a few, for ideas to make their way from laboratory to product. One way to look at that is that there is a fifteen year pipeline of ideas and technology. Only a few of these ideas will, in fact, become commercial, and we have no good way to predict which of them will be the most important. Thus, if one stops filling the pipeline, the effect on industry will not be immediately visible as it "drains" the pipeline, nor will the exact nature of the future impact be predictable. But that there will *be* an impact is an inescapable lesson of history.

As was noted in the recent (February 2005) Defense Science Board (DSB) Task Force on High Performance Microchip Supply:

> *"University and independent laboratory work has played and important*
> *role in microelectronic history in that it has sown the seeds for major*
> *technological shifts. … At a time when the effectiveness of conventional*
> *approaches to the extension of Moore's Law are nearing their end, new*
> *ideas are essential to continue the progress on which the industry and*
> *future military systems depend."*

Although this DSB report is focused on micro-electronics, much the same can be said for all aspects of information technology. At a time of growing global competition, DARPA's disinvestment in university-based, long-term research is, in my view, a risky game for the country.

Third, please permit me to vent an old annoyance. Information technology has become critical to virtually every agency of the federal government, and specifically to those that fund research – NASA, DoE, NIH, EPA, NOAA, etc. I believe it is fair to say that these agencies could not fulfill their primary mission without the information technology developed in the last 50 years. Yet none of these agencies has contributed significantly to the development of the basics underlying that technology. As concerned and unhappy as I am with the trends at the traditional funders of computer science, I am at least as much so with the complete absence of those other agencies that benefit enormously from computer science research!

Now let me turn to the question about the government's priorities. I suspect that the answer to this question by a set of randomly chosen computer scientists would vary enormously and correlate well with whether an individual researcher's interest was on today's "in list". My concern is less with what is on today's "in list" than with the frequency with which the list changes. As I tried to say in my previous testimony to this committee on the issue of cybersecurity, stability of funding is as important as its magnitude. Academic careers are built on a reputation for work done over decades. If the

perception is that an area is a "fad", it may attract a few weaker researchers, but the best researchers will migrate to where multi-decade support is probable.

I understand the desire for program officers and agency heads to "make their mark", but I think the most effective and profound change the government could make would be to ensure that any new programs last long enough to have an effect – to attract people, let them find their footing, have a real chance to succeed or fail, and produce real benefit to society! Such a move would both raise the bar on evaluation of new programs and create the stability that will ensure that the best researchers become involved.

To answer your third question -- as you might expect from my previous testimony to this committee[1], I am strongly in agreement with the recent PITAC report on Cybersecurity[2]. I am particularly pleased that they strongly identified the need for a better funded and stable program of long term basic research; as you will recall, that was what I also recommended. In my view, the dominant model of cybersecurity, namely a perimeter defense, is flawed and incremental patches to it will never result in the level of security we need for today's systems, much less the increased dependence we should expect for future ones. This is an excellent example where boldness and courage are needed, and hence where the perception of excessively low proposal success rates can have severe consequences! Their one recommendation that was not in my previous testimony concerns the need for coordination among the various agencies that fund cybersecurity

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[1] Testimony to the House Science Committee, *CYBER SECURITY: BEYOND THE MAGINOT LINE*, 10 Oct. 2001
[2] President's Information Technology Advisory Committee (PITAC), *Cyber Security: A Crisis of Prioritization*, February 2005.

research, and I concur that such coordination is needed. It is too soon to know what will

happen as a result of the report, but I hope it will be aggressively implemented.

Concerning PITAC – I believe it fulfils a unique and important role. Its reports on Health

Care Information Technology and Cyber Security have been extremely valuable, and I

expect their forthcoming report on Computational Science will be as well. So, from my

perspective it is important for PITAC to be re-chartered, but that clearly hinges on the

administration's perception of its utility, not mine. If it is re-chartered, I would like to see

PITAC tackle the broader issues that are the subject of this hearing, namely whether the

nation's overall information technology R&D investment appropriate for us to maintain

our lead in this critical field.

Thank you for the opportunity to testify on this important matter.